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Diachronic and Synchronic Tone Rules in the Etsakọ Verbal System: Some Theoretical Implications*

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There has been a good bit of discussion recently concerning the tonal system of Etaskọ[1], from both a diachronic (Elimelech 1974) and a synchronic (Elimelech 1978, Leben 1978, Clements 1979) perspective, in an attempt to account for some rather complicated synchronic tonal alternations, especially in the verb phrase. In this paper, I will attempt a diachronic account which is considerably more comprehensive than the previous account, as well as, I will argue, more plausible with respect to the areas which have been treated by Elimelech. I will then go on to examine the synchronic system which has resulted from the changes in question, arguing for an analysis which is different in theoretically interesting respects from all previously proposed analyses. Finally, I will pursue the implications of my diachronic and synchronic accounts for the relationship between synchrony and diachrony in tonology in general, especially with respect to non-segmental theories of tone. In particular, I will argue that there are certain kinds of synchronic tone rules that have no formal analogue in rules that express diachronic changes. Based on this discrepancy, I will argue that tonal "autosegments" (Goldsmith 1976) are actually mapped onto tone-bearing units, as originally proposed by Leben (1973), and not merely "associated with" them, as Goldsmith (1976) and most other current researchers--including Leben (1978)--maintain, and explore the consequences of accepting this proposal with respect to what Goldsmith calls "tonal stability". I will also argue that the synchronic system of Etsako provides good evidence against Leben's (1973, 1978) "Obligatory Contour Principle".

1. The data. I will be concerned here almost exclusively with the verbal system, especially the behavior of verb + noun object sequences, since the rest of the tonology appears to be fairly straightforward, even from a synchronic perspective[2]. The following data, all involving a third person singular pronominal subject, are taken from Elimelech (1978:85-107), with the exception of those followed by a question mark, which have been inferred from his rules and the behavior of similar forms. They represent the forms found in the "tenses" listed in the affirmative (I, II) and the negative (III, IV). The five nouns used for illustration have the forms: úts̀ádè 'put', àkpà 'cup', ọ̀gẹ̀dệ 'banana', àtásà 'plate', and úkpò 'cloth'; the verb stems which are representative of most of the one and two syllable verbs in the language, do not occur in isolation, but are presumably underlyingly dẹ́ 'buy' and kélé 'look for' (cf. the infinitive forms úd̀ẹ́mhì and úkélémhì), with the final vowel being "elided" before the vowel-initial object noun.

Table 1

	Present	Future	Past	Past Perfect	Habitual	Past Hab.
I.	ọ̀ dùtsádẹ̀	ọ̀ θá dùtsádẹ̀/ 'dútsádẹ̀	ọ̄ dútsádẹ̀	ọ̄ xè dútsádẹ̀	ọ̀ dútsádẹ̀	ọ̀ yá dùtsádẹ̀/ 'dútsádẹ̀
	dàkpá	dàkpá/ 'dàkpá	dâkpà	dâkpà	dâkpà	dàkpá/ 'dákpá
	dǫ́gẹ̀dẹ̀	'dǫ́gẹ̀dẹ̀	dǫ̂gẹ̀dẹ̀ ?	dǫ̂gẹ̀dẹ̀ ?	dǫ̂gẹ̀dẹ̀ ?	'dǫ́gẹ̀dẹ̀
	dá'tásà	'dá'tásà	dâtásà	dâtásà	dâtásà[3]	'dá'tásà
	dúkpò	'dúkpò	dúkpò	dúkpò	dúkpò	'dúkpò
II.	kèlùtsádẹ̀	kèlùtsádẹ̀	kélútsádẹ̀	kélútsádẹ̀	kélútsádẹ̀	kèlùtsádẹ̀
	kèlàkpá	kèlàkpá	kélâkpà/ kélàkpà	kélâkpà/ kélàkpà	kélâkpà/ kélàkpà	kèlàkpá
	kèlǫ́gẹ̀dẹ̀	kèlǫ́gẹ̀dẹ̀	kélǫ̂gẹ̀dẹ̀/ kélǫ̀gẹ̀dẹ̀ ?	kélǫ̂gẹ̀dẹ̀/ kélǫ̀gẹ̀dẹ̀ ?	kélǫ̂gẹ̀dẹ̀/ kélǫ̀gẹ̀dẹ̀ ?	kèlǫ́gẹ̀dẹ̀
	kèlá'tásà	kèlá'tásà	kélâtásà/ kélàtásà	kélâtásà/ kélàtásà	kélâtásà/ kélàtásà	kèlá'tásà
	kèlúkpò	kèlúkpò	kélúkpò	kélúkpò	kélúkpò	kélúkpò
III.	ǫ̂ dùtsádẹ̀	ǫ̂ θă dùtsádẹ̀	ǫ́wà dùtsádẹ̀	ǫ́wà xĕ dùtsádẹ̀	ǫ̂ dùtsádẹ̀	ǫ̂ yă dùtsádẹ̀
	dàkpá					
	dǫ́gẹ̀dẹ̀	Tone patterns in other tenses are same as in present for verb-object combinations				
	dá'tásà					
	dúkpò					
IV.	kèlùtsádẹ̀					
	kèlàkpá					
	kèlǫ́gẹ̀dẹ̀					
	kèlá'tásà					
	kèlúkpò					

Clearly, the tone patterns in the case of the present, future, and past habitual in the affirmative, and of all "tenses" in the negative, are not what one would expect on the basis of the infinitival forms of the verbs and the isolation forms of the nouns; but the remaining three tenses show precisely the expected behavior. Furthermore, those tense morphemes which have a surface segmental realization (as opposed to the purely tonal marking of the distinction between past and habitual in the affirmative) invariably show a rising tone in the negative, while the affirmative future and past habitual show high tone, and past perfect has low tone[4]. Note also that verb-object sequences show no tonal differences from tense to tense in the negative, unlike in the affirmative. It is these three sets of facts with which my discussion will be primarily concerned.

2. The diachronic origin of the alternations.

2.1. Elimelech's account.

Elimelech (1974) has proposed a rather sketchy analysis of the development of what he calls there the past, present, and customary constructions, where these correspond to his later past, present progressive, and habitual, respectively. His account posits earlier forms of the verb stems and nouns identical to the synchronic forms mentioned above, and high and low toned vowels as markers of the past and present tenses, respectively, with the customary having no overt marker. The development of the customary is then straightforward: the only change is the loss of the final vowel of the verb stem and the appearance of its tone on the following vowel (as predicted by the non-segmental theories mentioned above). The past is also fairly straightforward: after the loss of the vocalic marker of past tense, the stranded high tone is associated with the pronominal morpheme to its left, resulting in the rising-toned pronoun which is characteristic of the past tense. The stranded tone is associated leftward, rather than rightward, in order to prevent merger of the past tense with the customary. The posited changes are schematized in (1) and (2) where $\underline{\grave{v}}$ represents the pronoun, $\underline{\acute{v}}$ the past marker and $\underline{cv\acute{}}$ and $\underline{\grave{v}cv\grave{}}$ represent the verb stem and a low-low noun[5].

(1) Customary: $\grave{v} + \emptyset + cv\acute{} \div \grave{v}cv\grave{} > \grave{v} \quad + c\acute{\emptyset} + \grave{v}cv\grave{}$

$\qquad\qquad\qquad\qquad\qquad\qquad\qquad\qquad\qquad\qquad\quad$ v cvcv

(2) Past: $\grave{v} + \acute{v} + cv\acute{} + \grave{v}cv\grave{} > \grave{v} + \acute{\emptyset} + c\acute{\emptyset} + \grave{v}cv\grave{} > \check{v}\ cv\hat{}cv\grave{}$

The present tense is a bit more complicated, and Elimelech treats low-low nouns separately from the others, although the loss of the vocalic present marker and subsequent assignment of the stranded low tone rightward is common to the evolution of all examples. The suggested evolution of this construction with a high-low noun is that given in (3), where the second $\underline{\grave{v}}$ represents the present tense marker.

(3) $\grave{v} + \grave{v} + cv\acute{} + \acute{v}cv\grave{} > \grave{v} + \grave{\emptyset} + c\acute{\emptyset} + \acute{v}cv\grave{} > \grave{v}\ cv\check{}cv\grave{} > \grave{v}\ cv\hat{}cv\grave{}$

In the case of low-low nouns, and only in the case of such nouns (and low-low-low nouns), a change of "high tone spread" plays a role in addition to changes similar to those which occurred in the case of the past tense. Elimelech's account is schematized in (4), where the fourth stage is the output of "high tone spread".

(4) v̀ + v̀ + cv́ + v̀cv̀ > v̀ + v̌ + cv́ + v̀cv̀ > v̀ + cv̌ v̀cv̀ >

v̀ + cv̌ v́cv́ > v̀ + cv̌cv́ > v̀ + cv̀cv́

The changes postulated are for the most part quite plausible ones, and the basic insight about the earlier existence of vocalic tense morphemes is, I believe, correct in a broad sense. However, there are a number of problems with the details of this account. First of all, it is incomplete (although, to be fair, Elimelech did not intend a complete account), in that only three tenses and monosyllabic verb stems are treated; in particular, no negatives are considered, and the differences between the negative and the affirmative are among the most striking facts about the verbal morphology of Etsakọ. Secondly, as Elimelech (1974: 71) suggests, this account entails that at an earlier stage "the customary is the most basic [i.e., least marked--DGC] of the constructions considered..." Since it is the simple present which is typically unmarked (cf. Tiersma 1982 and the references cited there), an analysis which sets up another category as unmarked should be carefully scrutinized[6]. More serious are the numerous appeals to the functional notion of avoidance of merger, especially as a trigger for a phonological change. While it seems clear that otherwise expected changes can be **blocked** if they would result in merger (cf. for example, Campbell 1974a), a change which occurs solely for the purpose of preventing another change from causing a merger is attested nowhere else, to the best of my knowledge. Such a change appears to be a priori extremely unlikely, and I would like to suggest that this is not a possible mechanism of phonological change. Furthermore, the postulated change of "high tone spread"[7], which is said to have such a motivation, does not appear to be a "natural diachronic tone rule" in the sense of Hyman and Schuh (1974), unlike the other changes suggested (although synchronic rules of this nature are not uncommon). Thus, not only does Elimelech's account require a change which has an extremely questionable functional motivation, but the change required is itself not a natural one. Finally, the putatively functionally motivated direction of association of the tones stranded by vowel elision (leftward in the past tense, rightward in the present) violates in the case of the present tense, an apparent universal (Leben 1978, Clements and Ford 1979), whereby stranded tones are associated with the trigger of the rule which resulted in their being stranded.[8]

2.2. The present account.

It seems clear that the evolution of the Etsakọ tonal system cannot be exactly as Elimelech has suggested--even within the limited domain which he treats--and I will argue here for an alternative, and fuller, account. My account agrees with that of Elimelech as far as the reconstructed form of nouns and verb stems is concerned, and our accounts are also in agreement with respect to the reconstruction of the past tense morpheme. It also seems quite likely that Elimelech is correct with respect to his reconstruction of the "present progressive" (i.e., simple present--cf. note 6) morpheme, although a not terribly plausible alternative is discussed below

(cf. note 10). There is fairly good evidence, however, that there was in fact an overt habitual marker--as one would expect on the basis of the considerations presented above--namely a rising-toned vowel (probably *a̰--see below). These reconstructions, together with those concerning morphemes not considered by Elimelech, are summarized in (5), where v indicates a vowel whose quality is not reconstructible:

(5) present--*v̀; past--*v́; habitual--*v̰; future--*θâ; past perfect--*v́ xḛ; past habitual--*yâ; negative--*v́ (clause initial)

The evidence in favor of the low-high tone sequence as a mark of the habitual concerns the various kinds of aspectual (?) modifications of past tense. Assuming that such forms were once transparently past tense + some other morpheme, and that modern y- in the past habitual comes from earlier *i--presumably the past tense morpheme, which of course bore high tone--then considerable further reconstruction is possible. The past perfect seems to be readily interpretable in this way, with *xe marking "perfective aspect" and if the vowel of the past marker was *i and the habitual marker was *a, then after simplification of *a to *à when followed by the high-toned verb stem in accordance with the sound change posited in (6b), we get intermediate *í à, which after the devocalization of i alluded to above would yield the form given in (5). Given such a further reconstruction, we would have an explanation for the lack of a rising-toned pronoun in the past habitual, as long as devocalization (with concomitant association of the stranded tone to the following vowel, as required by the universal mentioned above) preceded the elision of tense morphemes required by (6e) below. In addition, of course, it would provide an earlier phonologically isolable past tense morpheme. One might question such a reconstruction on the basis of the behavior of the negative forms, since -wà, which appears to be the (negative) past tense morpheme--appearing in both the simple past and the past perfect--is not present in the past habitual negative. Thus, it could be argued, the lack of -wà in the past habitual negative indicates that the past habitual never was--at least insofar as internal reconstruction is able to ascertain--composed of phonologically isolable past tense and habitual morphemes. However, given the other oddities with respect to the negative in general, such as the presence of -wà in **any** of the tenses, as well as the lack of tense to tense tonal distinctions and the rising toned tense markers, the behavior of negative forms cannot be taken as very good evidence about the morphological composition, either diachronic or synchronic, of the corresponding affirmative forms.

The basic idea behind these reconstructions it that the unexpected tone patterns (i.e., those in the present, future, and past habitual, and in the negative) came about as the result of a rightward "spreading" of low tones from tense markers onto the verb stem together with a number of subsequent changes specified below. The reason why the past, past perfect, and habitual fail to show the effect of this spreading is the presence of a high tone--either as a simple high-toned vowel in the case of the past, or as part of a rising tone, as in the other two cases--which has the effect of blocking low-spreading. The habitual and past perfect are reconstructed with a rising tone, rather than with a simple high, in order to explain the differential behavior of these morphemes and the past tense morpheme with respect to the tone of the pronominal element. The rationale behind the

reconstructions should become clearer in the sketches of the diachronic development of various affirmative forms given in (7), where the effects of the sound changes given in chronological order in (6) are illustrated (and where '-' represents a morpheme boundary and tones joined by a ligature are attached to a single tone-bearing element).

(6) a. *L - H > L - LH

b. *$\widehat{\alpha H\ -\alpha H}$ (#) -αH > αH (#) -αH (i.e., *HL L > HL, *LH H > L H

c. *$\widehat{LH}$ > H (= 'H after H, due to the application of "downdrift" --cf. Churma 1982)

d. *L H> L $\widehat{LH}$ (morpheme-internally only)

e. "elision" of reconstructed v̲'s and final vowels of verb stems

(7) a. Pres.: *ò # ǹ - dé # útsádè — *ò # ǹ -dé # ògèdê

	dé̌	6a	dé̌
	dè̌	6b	———
———		6c	dé
———		6d	ě̀
	dŭtsádè	6e	dǫ̂gèdě̀

b. Fut.: *ò # Θâ - dé # útsádè — *ò # Θâ - dé # ògèdê

	dé̌	6a	dé̌
á	dè̌	6b	á
		6c	'dé
		6d	ě̀
———	dŭtsádè	6e	'dǫ̂gèdě̀

c. Hab.: *ò # v̌ - dé # útsádè — *ò # v̌ - dé # ògèdê

———		6a	———
v̌		6b	v̌
———		6c	———
———		6d	ě̀
ò	dútsádè	6e	dǫ̂gèdě̀

d. Pres.: *ò # ǹ - kélé # útsádè — *ò # ǹ - kélé # ògèdê

	ké̌	6a	ké̌
	kè	6b	kè
		6c	
———	lě	6d	——— lě ě̀
	kèlŭtsádè	6e	kèlǫ̂gèdě̀

e. Hab.:	*ọ̀ # v̌ - kélé # útsádè		6a	*ọ̀ # v̌ - kélé # ọ̀gèdê	
	v̌		6b	v̌	
			6c		
			6d		ọ̀gèdě̀
	ò	kélútsádè	6e	ọ̀ kélôgèdě̀	

The final forms are not those found synchronically, of course. One problem concerns the rising-falling tone on ọ̀gèdê produced by the operation of (6d), which should be simply falling. Incorrect rising tones due to (6d) are also found in the útsádè forms in (7a, b, d), where we should have for (7b) variants with either low or (downstepped) high, while for the others we should have only one possibility--low tone. The latter problem carries over to the synchronic analyses considered below, and further discussion will be postponed until then. As for the former, it can be handled by positing a subsequent change such as that given in (8):[9]

(8) *L $\widehat{\text{LHL}}$ > L $\widehat{\text{HL}}$

This is not at all implausible, given the highly marked nature of rising-falling tones. In fact, since such tones do not, to the best of my knowledge, occur at all on short vowels (although they, and even tones with four components--cf. Lovins 1971a--can be found on long vowels), (6d) may have been blocked by the presence of the falling tone. (If so, then of course (8) is unnecessary).

As far as the affirmative is concerned, only one further aspect requires comment.[10] Forms such as àkpà in the present, future, past habitual, and the negatives seem to behave quite anomalously with respect to the account suggested above. The expected development for the present tense is sketched in (9):

(9) Pres.:	*ọ̀ # v̀ - dé # àkpà			
		ė̀	6a	
			6b	
		é	6c	
			6d	
	ọ̀	dâkpà	6e	

This is not even close to being correct for the present (although it is precisely correct for the habitual!). If àkpà had had high, rather than low, tones prior to the sequence of events outlined in (6)--but only in the anomalous tenses--then something quite close to the correct output is obtained, as illustrated, again for the present, in (10):

(10) Pres.:	*ọ̀ # v̀ - dé # ákpá			
		ě	6a	
		ȅ	6b	
			6c	
			6d	
	ọ̀	dǎkpá	6e	

I will assume that changes which had the effect of converting low tones to high on nouns which bore only low tones in the present, future, and past habitual had taken place prior to the events in (6), although exactly what these changes were and why they took place is far from clear. The inappropriate rising tone will be treated, as before, as a synchronic problem. There is a generalization to be found concerning which tenses exhibit these changes: precisely those reconstructed with a low tone (either as a simple low tone or part of a falling tone) immediately preceding the verb stem. It seems almost as if, loosely speaking, this low tone, after having been spread onto the verb stem by (6a), "dislodged" the high tone of the latter, which in turn "dislodged" the low tones of these nouns. Even within such a figurative account, however, it is not clear why only nouns which bear exclusively low tones (and not, e.g., ǫ̀gȩ̀dȩ̂) participate in the "dislodging". Apart from such cases, however--which, it should be recalled, were not given a convincing account by Elimelech, either--all of the forms in the affirmative have been satisfactorily accounted for.

Let us now consider the negative forms. Given the reconstructed forms in (5), we would expect the verb-noun forms to show precisely the same tonal patterns as the correspnding affirmative forms, since the only difference is the presence of the negative morpheme, which is not adjacent to the verb-noun forms. Only in the case of present tense, however, are such forms found (although the future and the past habitual show one of the affirmative variants in the forms for which there is variation). Since the tone patterns found in the negative have been seen to be the result of a low toned present tense morpheme in the affirmative (note that they are identical to not only the negative present, but also the affirmative present), it is tempting to propose that the negative tone patterns are also due to the former presence of a low toned vowel which was part of a discontinuous negative morpheme and directly preceded the verb stem. The existence of the low toned -wà in the past and past perfect, furthermore, appears to support such a proposal, although it would be unclear why it surfaces in just these tenses.

In the case of the past perfect, however, this -wà is in the wrong place, since the tense morpheme, and not -wà, is adjacent to the verb stem. Furthermore, this proposal incorrectly predicts an initial downstepped high tone in the future and the past habitual, rather than the simple high tone actually found, as illustrated for àtásà in (11); where v̋...v̋ is the discontinuous negative morpheme:

(11) * v̋ # ǫ̀ # θâ # v̋ -dȩ́ # àtásà

ȩ̌ 6a
á 6b
'é 6c
ǎ 6d
ǫ̀ θá dâtǎsà 6e

Assuming that the falling tone on the future marker, like the contour tones found in the verb-noun combination, is simplified by a synchronic rule to high tone, we would get the incorrect ǫ̀ θá 'dá'tásà.

We must, therefore, find an alternative explanation for the negative forms. The only reasonable alternative appears to be analogical leveling

in the negative "superparadigm": using the morphologically least marked present tense as the basis for the analogy, Etsakọ speakers leveled out the tense-to-tense differences in verb-noun tonal patterns. (The result, in the case of the habitual, was the loss of the only thing that distinguished it from the present.) For some reason, it appears, the tone patterns on the verb-noun combinations were taken to be (part of) the means of indicating negation. Why this should be so is not clear, but there seem to be at least partial (synchronic) parallels in other languages; Larry Hyman and David Stampe (personal communication) have noted that in Haya and in Sora there is a similar striking reduction in tense-to-tense contrasts in the negative. It could well be that this synchronic impoverishment is due to the kind of diachronic development suggested here.

Only the rising tones on the negative tense morphemes remain unaccounted for. The future and the past habitual can be accounted for by positing the following change, which postdated those given in (6):

(12) $\widehat{HL}$ # H > $\widehat{HL}$ # $\widehat{LH}$

Note that it would not do to describe the change of H to LH as occurring after a L, since this would result in incorrect rising tones on tense morphemes in the affirmative. Nevertheless, the past perfect--where there is no preceding falling tone--has a rising tone. This is apparently another case of analogy. The result of these occurrences of analogy is that in present day Etsakọ, the negative is indicated in some cases (the future, past perfect, and past habitual) doubly redundantly--by the rising-toned tense marker and the verb-noun tone patterns, in addition to the falling toned pronoun/-wà.

To summarize, the account just described has several advantages over that of Elimelech. First of all, of course, it accounts for the development of a much greater number of forms, including the negative forms, which underwent a rather interesting leveling of tonal contrasts from tense to tense. Even in the case of the tenses treated by Elimelech, some improvement has been made, since no reference to functional factors is required in order to determine the direction of association of stranded tones--in violation of an otherwise valid universal (one which has a quite plausible motivation--cf. section 4). It also allows for a stage in the development of the language which conforms to Greenberg's (statistical) universal concerning the unmarked tense. Furthermore, the one kind of case which was not given a satisfactory treatment--all-low-toned nouns in the problematic tenses--was not satisfactorily accounted for by Elimelech. An explanation for the development of these must apparently await further comparative evidence; at any rate, they have so far successfully resisted both Elimelech's attempted internal reconstruction and my own.

3. The synchronic system. In this section, I will examine the tonology of the Etsakọ verbal system from a synchronic perspective. After pointing out inadequacies of previous treatments, I will propose a reanalysis which avoids these problems, and requires a rule which is of considerable theoretical interest, since it violates what Leven (1973) calls the "Obligatory Contour Principle".

3.1. Elimelech's analysis. Elimelech's account assumes a nonsegmental theory of tone which does not allow for the possibility of mapping a single

tone onto more than one tone-bearing unit. The underlying representation of <u>útsadè</u>, for example, would be:

(13) H H L
| | |
utsadè

Tones are mapped onto tone-bearing units by a (presumably universal) set of rules which need not concern us here, since his system is, at least for present purposes, descriptively equivalent to a segmental theory of tone, with the exception that tones remain when the segment that bear them are lost. For reasons of space, I will discuss his analysis using such a quasi-segmental system.

The core of Elimelech's treatment of the verbal system is a set of four tone rules which apply in the present, future, and past habitual tenses, and in the negative (pp. 87-9). I give below the rephrasal of these rules given in Leben (1978:183):

(14) a. $[X\ H]_V \quad [L_1\ ]_N$
$\downarrow$
H_1

b. $[\ H\ ]_V \quad [H\ H\ X]_N$
$\downarrow$
L

c. $[H\ H]_V \quad [H\ X]_N$
$\downarrow\ \downarrow$
L L

d. $[H\ H]_V \quad [L\ X]_N$
$\downarrow$
L

(14a) is intended to have the effect of raising all of the L's in a noun object which consists entirely of low tones (although it is not clear, without some kind of convention for reading such a notation, that this rule would not also affect the initial L's in, say, a (LLH noun); (14b) lowers the H of a monosyllabic verb stem when it is followed by a noun which begins with at least two H's; (14c) lowers both H's of a bisyllabic verb stem when a noun with initial H follows; and (14d) lowers the H of only the first syllable when a noun with initial L follows. He also makes use of a set of independently motivated "optional" rules (cf. note 2 and below) which have the following effects (cf. Elimelech 1978:110 for a summary), where <u>D</u> represents downstepped high tone, and <u>H*</u> non-downstepped high:

(15) a. $\widehat{LH}$ H* → L H
b. $\widehat{HL}$ L → H L
c. H $\widehat{HL}$ → H L
d. $\widehat{LH}$ → H
e. $\widehat{HL}$ D → H D[11]

I give below several derivations which illustrate the role played by these rules in Elimelech's system:

(16) a.
ọ́ dé útsádẹ̀	(present)
dẹ́	14b
d´	elision
dŭtsádè	Mapping
ọ́ dùtsádẹ́	15a

b.
ọ́ dẹ́ àkpà	(present)
ákpá	14a
dè	14b
d´	elision
dăkpá	mapping
ọ́ dàkpá	15a

c.
ọ́ dẹ́ úkpò	(present)
d´	elision
ọ́ dúkpò	mapping

d.
ọ́ ∅ dẹ́ àkpà	(past)
´	elision
ŏ dâkpà	mapping

e.
ọ́wà dẹ́ àkpà	(past neg.)
ákpá	14a
dẹ̀	14b
d`	elision
dăkpá	mapping
ọ́wà dàkpá	15b

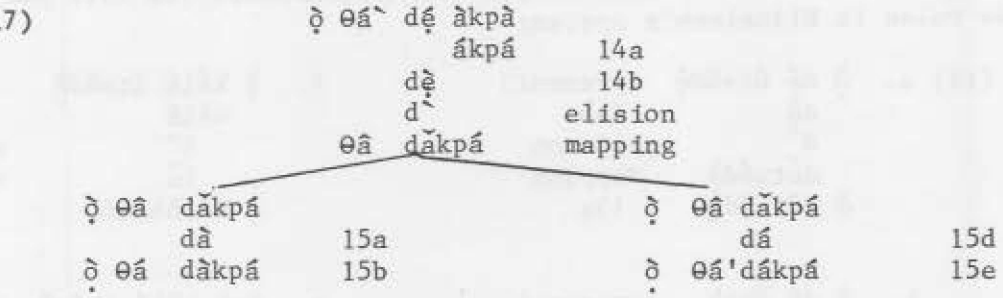
f.
ọ́ kélé útsádẹ̀	(present)
kèlè	14c
l`	elision
lŭ	mapping
ọ́ kèlùtsádẹ̀	15a

g.
ọ́wà kélé àtásà	(past neg.)
kèlé	14d
l´	elision
lâ	mapping
ọ́wà kèlá'tásà	15e

Elimelech's analysis succeeds in generating all of the attested surface forms from the input forms he posits. Moreover, the fact that it also generates forms other than those given as surface forms by Elimelech (due to the supposed optionality of the rules in (15)) is common to both Leben's analysis and my own, and I will not consider this matter further; apparently, the only way of preventing overgeneration under **any** analysis is extensive appeal to grammatical conditioning with respect to whether a given rule in (15) is in fact optional, obligatory, or not applicable at all. There are slightly more complicated cases of a similar nature which are unique to Elimelech's anlaysis (although Leben's account has problems of a different nature in these cases--see below). These problematic cases involve nouns whose first two syllables bear high tone, whether underlyingly or as the result of the operation of (14a), in the future and past

habitual tenses in the affirmative. Elimelech's account in the case of the future with <u>àkpà</u> as object (p. 95) is sketched below:

(17)



That is, after the appication of all obligatory rules, the "optional" rules are divided into two sets of two rules each. Within each set, the rules apply obligatorily, but either set may be selected to apply; however, one, and only one, of the sets must be selected. In other words, either both (15a) and (15b) must apply, or both (15d) and (15e) must. Clearly, this anlaysis is highly unusual, and I know of no theory of rule interaction which would permit it; some reanalysis in this respect is obviously in order.

A further, and probably more obvious, problem with this account is the fact that it posits **four** separate rules which refer to an extremely unusual set of non-phonological categories--present, future, and past habitual tenses, and **all** tenses in the negative. Clearly, an account which does not require such extensive reference to an obviously disparate class of grammatical categories such as this is, ceteris paribus, to be preferred over one that does.

3.2. <u>Leben's analysis</u>. Leben's account is framed within a theory which is in some respects just the opposite of that implicitly accepted by Elimelech. While in both theories, tone and "ordinary" segmental features are represented on separate tiers (in the sense of Goldsmith 1976), Leben's theory prohibits sequences of like tones in the underlying representation of a morpheme, and contains a "convention on tone melodies" (Leben 1978:181)) which simplifies such a sequence to a single tone whenever it arises in the course of a derivation. As a result, he reformulates Elimelech's rules (14b-d) as follows (p. 185):

(18) $\begin{matrix} V \\ | \\ T \end{matrix} \rightarrow \begin{matrix} V \\ | \\ L \end{matrix} \; / \; \_\_ C_o \;] \; \begin{matrix} V \\ \{ N \; Pref \} \end{matrix} \; [+ N] \; H / Y$

As in Elimelech's analysis, this rule must be restricted to applying in the present, future, and past habitual tenses, and in the negative. It has the effect of lowering high tones when they occur on a vowel in a verb stem or noun prefix and are followed by a high toned vowel. Together with rules (14a) and the equivalent of (15a, b), most of Elimelech's data can be generated from essentially the same underlying forms as in Elimelech's

account. The following derivations illustrate the effects of these rules in the present tense:

(19) a. ò dɛ́ ò̩gɛ̀dɛ̀

	14a
ò dò̩gɛ̀dɛ̀	elision
ò̩ dɔ́gɛ̀dɛ̀	15b
	18

b. ò kélé útsádɛ̀

ò kélútsádɛ̀	
ò̩ kèlùtsádɛ̀	

This account does in fact handle the present tense forms without difficulty. Furthermore, having replaced (14b-d) with a single rule must undoubtedly count in its favor. There are, however, some forms which are problematic. Some are rather trivial, and can be handled by making use of the remaining rules in (15), but the variant pronunciations in the future and past habitual appear not be amenable to treatment by simply adding another rule, although one of the variants is indeed generated (as long as (15e) is ordered before (18)), as indicated below:

(20) ò̩ θâ dɛ́ àkpà

ákpá	14a
dákpá	elision
	15b
ò θá'dákpá	15e
ò̩ θá dàkpá	18

This assumes that (18) applies to downstepped high tones. If it does not, then ò̩ θá 'dákpá would be derived--the other variant--but unless it does and is made optional, only one of the variants can be derived. Since there is already an obligatory/optional/inapplicable problem with respect to the rules in (15)--for Leben's analysis, and my own (see below), as well as for that of Elimelech--it might be supposed that making this rule optional would not create any really new problems. However, the rules in (15) are independently motivated as being optional by variant pronunciations in the nominal system, and this clearly cannot be the case for (18), which applies only in the specified tenses. In fact, this unusual morphological restriction is a good indication that this rule **cannot**--because of the semantic task it performs--be optional. It is also worth nothing that this restriction makes it clearly a Stampeian rule (and not a "natural process"), which would entail in Stampe's framework (cf. Stampe 1973, Donegan and Stampe 1979) obligatory application. In addition, making (18) optional would incorrectly entail in the case of bisyllabic verb stems the existence of variants such as present tense *ò̩ kélútsádè--an especially striking demonstration of the failure of this suggestion, since this is precisely the (only) form found in the habitual.

One final, though rather minor, problem with this analysis is the further reference made to disjunctive morphological categories in rule (18). This rule thus requires reference not only to the curious set of tenses required by Elimelech's rules, but also to the equally curious set consisting of verbs and noun prefixes. In the next subsection, I will present an analysis which has neither this drawback nor the others pointed out with respect to Elimelech's and Leben's analyses.

3.3. The present analysis. Like both of the previous analyses, mine assumes that tones and segmental features may be represented underlyingly on separate tiers. It also makes crucial use of the possibility of violating Leben's Obligatory Contour Principle (cf. section 3.2).

The replacement for Elimelech's rules (14b-d) and Leben's (18) is suggested by the output of the sound changes proposed in (6). Recall that monosyllabic verb stems ended up with either a high tone or a low tone in the unusual tenses, depending on whether (6b) or (6c) simplified the rising tone that resulted from the occurrence of (6a); bisyllabic verbs were invariably low-rising. Since the synchronic analogues of (6b) and (6c) are independently motivated by alternations in the nominal system (cf. rules (15a, d)), a rule which yields a rising tone on monosyllabic verb stems and on the final syllable of bisyllabic stems (with a low tone on the first syllable) will be able to account for the alternations in question. I suggest the following, which applies in the present, future, and past habitual, and in the negative:

(21) $\begin{matrix} X \\ | \\ T \end{matrix} \rightarrow \begin{matrix} X \\ \\ L\ L\ H \end{matrix} \quad / \_\_ \ C_o]_V\ N [$

A few comments about the formulation of this rule are in order. Note first of all that the output contains two consecutive low tones--a clear violation of the Obligatory Contour Principle. Note also that the tones in the output are not linked by "association lines" to any segmental material. This is necessary because the rule is intended to be applicable to both monosyllabic and bisyllabic verb stems; the required association lines will be provided by the universally applicable "Well-Formedness Conditon" (WFC) proposed in Goldsmith (1976).[12] It is also worth pointing out that this rule is considerably simpler than Leben's rule (18), since it requires reference neither to noun prefixes nor to a following high toned vowel in a noun, and that it is clearly simpler than the corresponding three rules in Elimelech's analysis.

Before illustrating the application of this rule, I would like to give some attention to the rules in (15), particularly with respect to the future and past habitual forms, which have been seen to be problematic in both Elimelech's and Leben's analyses. In order to avoid the problems with Elimelech's account, it is desirable to collapse (15b) and (15e), if possible. It is indeed possible to do so, since the falling tone is simpified to high in both cases and is required to be followed by the only tones which could possibly follow it (cf. note 11) when the two rules are taken together. They can thus be replaced by the single rule (22):

(22) $\widehat{HL}\ T \rightarrow H\ T$

(The possibility of collapsing these two rules was apparently overlooked by both Elimelech and Leben, since they both collapsed (15b) with another rule, (15a), by means of alpha variables.) Rules (21) and (22), together with (14a) and (15a, c, d) (ignoring, of course, the optional/obligatory/-inapplicable problem), can account in a plausible way for all of the forms in Table 1, as illustrated below (where the optionaity or obligatoriness of rules in a given morphological category is indicated in parentheses):

(23) a.

ò dé̤ ò̤gè̤dê	(present)	
———	14a	
dé̤	21	
dò̤gè̤dê	elision	
ò̤	22 (oblig.)	
ò dò̤gè̤dê	15a (oblig.)	
	15d (oblig.)	

b.

ò̤ kélé útsádè̤	(present)	
———	14a	
kèlĕ	21	
kèlŭtsádè̤	elision	
———	22 (oblig.)	
ò̤ kèlútsádè̤	15a (oblig.)	
———	15d (oblig.)	

c.

ò̤ θâ dé̤ útsádè̤	(future)	
———	14a	
dĕ	21	
dŭtsádè̤	elision	
θá	22 (oblig.)	
(ò̤ θá dùtsádè̤)	15a (opt.)	
ò̤ θá'dútsádè̤	15d (oblig.)	

d.

ò̤ θâ kélé útsádè̤	(future)	
———	14a	
kèlĕ	21	
kèlŭtsádè̤	elision	
θá	22 (oblig.)	
ò̤ θá kèlùtsádè̤	15a (oblig.)	
———	15d (oblig.)	

e.

ò̤ θâ kélé úkpò	(future)	
———	14a	
kèlĕ	21	
kèlŭkpò	elision	
θá	22 (oblig.)	
	15a (oblig.)	
ò̤ θá kèlúkpò	15d (oblig.)	

f.

ò̤ dé̤ àkpà	(habitual)	
———	14a	
———	21	
ò̤ dâkpà	elision	
———	22 (blocked)	
———	15a (?)	
———	15d (?)	

In the present (23a, b), the post-elision rules are all obligatory, and the rising tone created by the application of (21) is correctly simplified to low when a high tone follows and to high otherwise. The same is true with respect to the future (23c-e), except that (15a) is optional when the verb stem is monosyllabic (but obligatory when it is bisyllabic). I am not at all happy about this consequence of my analysis, but it is at least an improvement over the previous analyses in that it does in fact generate both variant forms, unlike Leben's, and does not require the strange mode of rule application illustrated in (17), as Elimelech's analysis entails. In any event, a parallel kind of situation must apparently be recognized with respect to (15c) in the past tense for all three analyses where this rule must be optional in the case of bisyllabic verbs, but blocked for monosyllabic verbs. If any one of these analyses is correct, then, this kind of formal mechanism must be recognized in phonological theory.

It might be suggested that all of these analyses are in fact incorrect by virtue of being excessively abstract, and that a more surface-oriented approach is required. Such an approach might make use of the rules given below in the troublesome tenses:

(24) a. $H]_V \rightarrow L$

b. $H\ H...]_N \rightarrow L\ H...$

c. $L_1\ \#]_N \rightarrow L\ H_1$

d. $L_2\ H...]_N \rightarrow H\ L\ H...$

e. $L\ H...]_N \rightarrow H\ D...$

Under this approach, the tone patterns on the verb-object combinations would be generated directly by first converting the tone of the verb stem to low (24a), and then altering the first part of the tone pattern of the noun in accord with the other rules in (24). If elision is formulated so as to delete the tone on the final vowel of the verb stem, roughly as in (25), and applies prior to the rules in (24), derivations such as those in (26) will result:

(25) $\begin{matrix} V \\ | \\ T \end{matrix}\Big] \rightarrow \phi\ \#\ \_\_\ [V$ Noun

Verb

(26) a. o # de # utsade # → (25) o # d # utsade → (24a) (inapplicable)

L # H # H H L # L # ϕ # H H L

→ (24b) o # d # utsade → (WFC) o # d # utsade

L # # L H L L # # L H L

b. o # kele # akpa # → (25) o # kel # akpa → (24a) o # kel # akpa

L # H # L # L # H # L L # L # L

→ (24c) o # kel # akpa → (WFC) o # kel # akpa

L # L # L H L # L # L H

This analysis generates precisely the same outputs as Leben's analysis; as such, of course, it fails to generate the second variant in the cases in the future and past habitual where there are alternative surface forms. One way of accounting for these forms within this approach is to posit an optional rule which follows those in (24), but precedes (22), and raises noun-initial low tones to high in the future and past habitual--but only for monosyllabic verb stems (it would have to be inapplicable in the case of bisyllabic verb stems). The necessity for making this rule optional (sometimes) makes it almost as suspicious as the corresponding "optional" rule in Leben's analysis, since, although it does not have quite as dramatic an effect as the latter rule, it too is subject to unusual morphological conditions on its applicability. Thus, the only analyses of those discussed which can account for the variant forms in the future and past habitual by means of an optional rule of the type likely to be optional are the first analysis discussed in this subsection and Elimelech's original analysis. More generally, **any** analysis which has this property must apparently recognize an intermediate stage with a rising tone on noun-initial vowels in such cases.

It is of considerable theoretical interest that all four of the analyses discussed here require a kind of constraint on the mode of application of a rule (i.e., whether it is optional, obligatory, or inappli-

cable) which is, to the best of my knowledge, unparalleled in descriptions of other languages, in that all require reference to the number of syllables in the verb stem in order to determine the mode of application of at least two rules (which, moreover, affect tones associated with a vowel that is part of a noun). I can conceive of no alternative analysis in which such requirements are unnecessary.

4. Implications. In this section, I would like to discuss briefly the implications of the preceding discussion concerning the relationship between diachronic tonal changes and synchronic tonal systems, and some related issues.

First of all, it should be clear that doing an internal reconstruction of the type performed in section 2.2 can have considerable heuristic value with respect to discovering a possible synchronic rule; rule (21) is in fact the result of a consideration of various stages in the proposed diachronic development. The synchronic account does not mirror exactly the diachronic account, however, and in this particular case it is impossible in principle that it could, due to the diachronic occurrence of an analogical leveling which cannot be incorporated into a synchronic system of phonological rules. This extension of the tonal alternations to all tenses in the negative also makes it rather unattractive to posit an abstract underlying "floating" low tone (one not associated with any vowel) as a marker of present tense: while one could make the synchronic analogue of the diachronic account work in the case of the present tense, similarly positing an abstract floating low would not work in the negative, for the same reason that attempting to attirubte the diachronic developments in the negative to the infuence of a lost low toned vowel did not (see section 2.2). Thus, in order to include both the present tense and the negative in the same synchronic generalization, it is necessary to refer to morphological categories, and not a low tone, as the trigger for the relevant rules--as was in fact done in all of the synchronic analyses discussed above. Furthermore, since such morphological conditoning is required, there is no good reason for attempting to find phonetically plausible rules--and, again, all of the analyses considered are similar in not requiring phonetic plausibility in the cases at issue. Etsako thus appears to provide good evidence that even a fairly abstract synchronic phonological analysis can differ quite strikingly from a diachronic account of the same facts, even if the latter is arrived at solely on the basis of data avilable to the synchronic analyst.

The second issue to be considered here is the apparently diffrent nature of the rules which express diachronic changes and those that account for synchronic alternations. All of the synchronic analyses considered contain rules which change tones associated with two or more different vowels (cf. rules (14a, 24c), for example); the diachronic account of section 2.2 incorporates no such rules. The latter fact is no accident; a deliberate attempt was made to include only phonetically plausible rules (i.e., "diachronically natural rules" in the sense of Hyman and Schuh 1974). The rationale behind this attempt is really nothing more than the traditional assumption that sound changes occur for a good reason--they are a response to some kind of physiological difficulty, either articulatory or perceptual. It is hard to see how changing a sequence of low tones to high, for example, could be interpreted in this way. The changes posited in section 2.2, on the other hand, are straigtforwardly interpretable (with the exception of "elision"--see below) as cases of either "horizontal

assimilation" (Hyman and Schuh) or contour tone simplification, both of which have a clear articulatory motivation. Diachronic rules which do not have a physiological motivaton are suspicious because in the realm of tone, as in other areas of phonetics/phonology, they explain nothing (though they may provide quite elegant statements about cumulative changes that have occurred from one stage in the history of a language to another).

It might be suggested that, however desirable a priori this assumption about the nature of sound change might be, the evolution of nouns which bear exclusively low tones in the problematic tenses indicates that it must be abandoned. (Recall that a change which, for reasons not explained, converted **all** of the low tones in such cases to high prior to the operation of the changes in (6) was required.) If tone is represented on a separate tier, and a single tone may be associated with more than one tone-bearing unit, then a change of (what appears to be) a sequence of tones is quite easy to state; the simple diachronic rule given in (27) would have the apparently impossible effect illustrated in (28):

(27) L > H / H __

(28) 

Given the input representation in (28), such a change can be interpreted simply as the total assimilation of one tone to a neighboring tone. But recall that there is, at least in the case of Etsakǫ, more to it than this, since grammatical conditioning would have to be added to (27) in order to restrict it appropriately. And even further restrictions would have to be placed on this rule, since only nouns composed exclusively of low tones (but not, e.g., L L H nouns--cf. note 7) were affected by the change in question. Even if such an approach were adopted, then, the rule required would not be a simple one; in view of this fact, abandoning an otherwise well-supported conception of the nature of sound change seems clearly to be insufficiently motivated.

If there is in fact a diachronic-synchronic asymmetry of this sort, then of course an explanation for it is in order. I would like to suggest that the relevant explanation can be found in Leben's (1973) proposal that tones, although represented underlyingly on a separate "suprasegmental" (to use his term) tier, are at some point in a synchronic derivation "mapped onto" tone-bearing units and so become features of them on a par with "ordinary" segmental features. As long as we assume that sound change can affect only post-mapping representations (a very conservative assumption, since there is good evidence that sound change affects only phonetic representations--cf. Jeffers 1977), the asymmetry in question follows naturally.

There are other advantages to accepting this proposal, as well. First, as Leben points out, it implies that all rules that make no mention of associated segmental material will precede those that do; I know of no counterexamples to this prediction. Secondly, making a distinction between pre- and post-mapping rules allows for the possibility that there are other characteristic differences between these two types of rules. It seems that this is in fact the case, in that the former are invariably obligatory (cf. (14), (18), (21), and (24) above), while the latter may be either obliga-

tory or optional. Leben's proposal about mapping thus appears to be quite strongly supported, since there are three universal properties of tone rules which otherwise appear to be quite unrelated, but which can be seen either to follow from this proposal or to be naturally statable in terms of it.[13]

Despite the advantages of this proposal, however, there are also some apparent disadvantages. Probably the most glaring problem is the existence of what Goldsmith calls tonal "stability", which refers to the fact that tones frequently are not lost when the vowel with which they are associated is lost (as in the elisions discussed in sections 2 and 3)--both in synchrony and diachrony. If tones are not features of vowels at the level at which the loss of the vowel occurs, then this is just what would be expected. But the mapping proposal requires that they be features of vowels at the level which is affected by sound change, so another explanation for stability must be found if this proposal is to be maintained. It should be noted that there is further reason to seek, if not a different explanation, at least a deeper one, since the universal concerning the direction of reassociation of (synchronically) "stranded" tones mentioned in section 2 does not appear to follow from rejecting the mapping proposal alone. Moreover, underlyingly "floating" tones, which presumbly arise from the diachronic analogue of the synchronic process in question (cf. Hyman 1978, Hyman and Tadadjeu 1976), appear to behave differently than we would expect on the basis of this universal, since they do not always surface (when they do) as being associated with the vowel which historically conditioned the "floating" of these tones (cf. Hyman and Tadadjeu 1976, Clements and Ford 1979).

The basis for the explanation to be argued for here is that segments are **not** deleted (at least in diachronic change and in the case of Stampeian natural processes in synchronic systems); rather, they are assimilated, either partially or totally, to neighboring segments.[14] In the case of the "elision" of vowels discussed above, the full diachronic sequence of events would be as sketched below:

(29) a. (partial) assimilation of one vowel to another with respect to all oral cavity features;
b. conversion of the resulting sequence of (nearly) identical vowels to a long vowel (but retaining the tonal features of the formerly distinct vowels);
c. shortening of the long vowel.

Schematically, if the first vowel bears high tone and the second low tone, the development is that given in (30):

(30) $\acute{v}_i\ \grave{v}_j > \acute{v}_j\ \grave{v}_j$ (or $\acute{v}_i\ \grave{v}_i$) $> \hat{v}_j\text{:} > \hat{v}_j$

If the assimilation in (29a) is total, and not only partial, in that tonal features are also assimilated, the result is "deletion" of the tone borne by the "deleted" vowel.[15] This phenomenon is also occasionally found, although partial assimilation seems to be more common--just as assimilation with respect to point of articulation only (which is, in essence, exactly what is happening here) is more common than total asimilation of a nasal consonant to a neighboring oral stop.

With respect to synchronc alternations, it is frequently the case that only the input and the output of (29) are readily identifiable and it is

cases of this nature that Goldsmith uses as the basis for arguments which depend on "tonal stability". But the intermediate stages required by this proposal are also frequently to be found. An optional synchronic rule of vowel shortening (i.e., process (29c)) has been proposed for Etsakọ by Elimelech (1978:33), for example, and cases which show the operation of both (29a) and (29b) are commonplace (cf., for example, Welmers 1973:41-2, Elimelech 1978:22, and many of the papers in George 1972), though often (mis)described as cases of "compensatory lengthening".[17] It is not crucial for present purposes that the output of (29a) exist as a stage independent of the output of (29b), and although I believe that there is a fair amount of evidence that this is in fact the case, I will not pursue the matter here.

Further evidence for this account of the existence of stability comes from phenomena which are not at all expected, given only the formal separation of tones and segments. Consider, for example, the following often cited Lọmọngọ example, whose theoretical interest was first noticed by Lovins (1971a, b):

(31) bàlóngó bǎkáé → bàlóngâkáé 'his book'

Although I know of no explicit proposals concerning the autosegmental representation of the input form in this example, (32) is the most obvious candidate:

(32)


"Deletion" of the second b and the word-final o (and its association line; as Clements and Ford 1979: 195n point out, this "follows from the binary nature of the association relation") would then yield:

(33)


This is incorrect, since the H formerly associated with the deleted o should show up on the following a. Nothing in autosegmental theory as formulated by Goldsmith (1976), however, indicates that it should. Neither does the proposal of Clements and Ford (1979: 207n) that an element "set afloat" by deletion of an associated segment reassociates to the segment that conditioned this deletion, since the H of the lost o has not been set afloat. It is true, of course, that having two different H's associated with the two o's would result in a H which has been set afloat, and therefore appropriately reassociated to the a, but given the lack of any independent motivation for such a representation, suggesting this as an explanation for these facts is clearly ad hoc. Leben (1978: 182), who is apparently the only one who has recognized the problematic nature of such cases, even given Clements and Ford's proposal, suggests that association lines are **not** deleted when one of the elements they associate is lost, and that "these lines are transferred to the segment that occasions the deletion". But this entails the existence of "association" lines that

don't associate anything (cf. Clements and Ford's position cited above). Thus, autosegmental phonology has so far failed to provide an adequate account of some aspects of tonal stability--which is claimed to provide strong support for the theory.

The assimilation proposal, on the other hand--which depends neither on tones being set afloat nor on association lines that associate something with nothing--provides a straightforward account. The ɔ̲ assimilates (after "loss" of the b̲) to the following a̲ with respect to oral cavity features only; the tone necessarily remains because it was not assimilated along with the other features (If it had been assimilated, it would have given the appearance of also having been deleted.) Thus, not only is this proposal plausible in its own right, as evidenced by the independent existence of the stages required to give the appearance of vowel deletion, it also provides an **explanation** for a phenomenon which otherwise cannot even be given a reasonable formal characterization.

It might be questioned whether all synchronic cases of apparent vowel loss should be characterized as the synchronic analogue of the diachronic account just proposed. It is quite likely that not all of them should be; in Etsakọ, for example, whether the first or second vowel of a sequence is lost depends on morphological information, which at least suggests that the elision rules no longer correspond to the diachronic processes that brought them about. It is not unlikely, that is, that there are quite genuine cases of deletion rules ("rules" in Stampe's sense--i.e., not natural processes) in synchronic phonology. If this is so, however, then we are very much back where started from as far as an explanation of stability phenomena in such cases is concerned, although as long as vowel deletion precedes tonal mapping there is at least a reason why tones are not **always** deleted. Cases such as the Lomongo example discussed above remain problematic, however, unless we can somehow guarantee that representations like (32) are never the input to rules of vowel elision (since if two H's--one for each of the last two vowels in bàlɔ́ngɔ́--are present, then the second will be set afloat by elision and reassociated by Clements and Ford's convention).

There is a fairly trivial way of doing so--by requiring that a single tone be associated with only one tone-bearing unit. Such a constraint seems to be presupposed by Elimelech in his treatment of Etsakọ, and, despite the fact that all other investigators seem to assume that it should not be maintained, I feel that it warrants serious attention. In addition to allowing for a characterization along the lines of Clements and Ford of the full range of synchronic (Stampeian rule) facts it constrains the theory of tonology in an interesting, and apparenty appropriate, way. Within Goldsmith's (1976) framework, there is a formal difference between the two representations given in (34):

(34) a. 

b. 

It has been argued that such a difference is necessary. Cheng and Kisseberth (1981), for example, have proposed a tone rule which depends on the possibility of making such a distinction; Odden (1982), following Goldsmith, proposes that this distinction has a phonetic correlate, with (34a) corresponding to what is traditonally termed a sequence of high and downstepped high tones, and (34b) to a sequence of simple high tones; and

Goldsmith (who refers to downstepped high tones as "mid") makes use of a similar distinction concerning the association of mid tones to "explain" why a mid that follows a mid has a lower pitch than the preceding tone. Analyses which depend on the existence of both such representations are rather rare, however (I know of no others), which at least suggests that this distinction is of questionable validity. In addition, such analyses require something which is very closely related to (and very much in the spirit of) what Kiparsky (1973) calls the "diacritic use of phonological features" in his argument against excessive abstractness in phonology. The abstractness required in analyses of this sort is perhaps sufficient reason for rejecting the tonal distinctions in question, but there is additional counterevidence as well. Neither Goldsmith nor Odden explains or even mentions the fact that no similar distinction with respect to low tones (and, in Goldsmith's case, high tones as well) plays a role in the language being described. If such a distinction is legitimate, then the total lack of any motivation for it in the tones not involved in downstep would be quite unexpected; if it is being used essentially as a diacritic to indicate downstep, on the other hand, this is not at all surprising.

There are, of course, arguments in favor of the position that a single tone can be associated with more than one tone-bearing unit; I will now attempt to counter these arguments. The first (cf. Leben 1973 and the references cited there) has to do with the putative existence of a restricted number of "tonal patterns" in a number of languages which occur in morphemes irrespective of the number of tone-bearing units they contain. Perhaps the paradigm example of a language of this type is Mende, in which patterns such as L H L--but not, e.g., H L H--are said to exist. That is, while there are monosyllabic morphemes with a triple L H L contour, disyllables with a low-falling sequence, and trisyllables with a low-high-low sequences, the corresponding H L H patterns, Leben claims, do not occur. But the latter do occur, at least for morphemes with more than one syllable (cf. Dwyer 1978: 184-5), and recent borrowings seem to show no tendency whatsoever to acquire the "permissible" tonal patterns (Dwyer 1978: 192); the apparent existence of such a restriction is explained by the occurrence of a double accident, one of history (Dwyer 1978: 185-91) and one of data sampled by Leben. Interestingly, languages claimed to have such restrictions (aside perhaps from "pitch accent" languages such as Japanese) always seem to have exceptions to them, even in cases where these patterns provide no support for allowing tones to be associated with more than one syllable (cf. Edmonson and Bendor-Samuel 1966 for an explicit statement concerning the existence of such exceptions in Etung, another often cited language which is claimed to have tone patterns that are associated with a unit larger than the tone-bearing unit).

Furthermore, in some cases where it is fairly clear that tonal patterns whch do not change depending on the number of tone-bearing units involved are needed--because they have a morphological function--these patterns of necessity have sequences of like tones. Examples of this include the Etsakọ rule (21) discussed above, and the tonal tense distinctions in Tiv (cf. Goldsmith 1976). In the absence of any genuine (nonacentual) examples of languages that require tones to be associated with more than one tone-bearing unit, it can safely be concluded that arguments of this sort are without basis.

Leben (1978) has given two further arguments in favor of this position, however. First, in Etsakọ, there is a rule which raises low tones to high in "associative" constructions. Leben formulates this rule (p. 181) as

follows (where "A" represnts the associative morpheme, which is not realized on the surface):

(35) L → H / ___ A

If tones can be (or must be, for Leben) associated with more than one vowel, then phonetic sequences of low-toned vowels in other contexts should show up as sequences of high-toned vowels in the associative. this is exactly what occurs, as illustrated in (36).

(36) a. uno A odzi (35)→ uno A odzi
H L H H H H

b. amę A eθa → amę A eθa
L L H L

As Leben points out (p. 182), it is possible to account for these facts even if we do not accept this position by rewriting (35) as (35'):

(35') $L_1 \rightarrow H_1$ ___ A

However, Leben continues, such an analysis "would yield no prediction, on the basis of [facts such as (36a)], that this process would also apply in [cases such as (36b)]" (pp. 182-3). In fact, such a prediction, though correct in this case, should not be made, since it gives incorrect results in other cases--even within Etsakọ itself. Recall that rule (14a) above affects nouns which contain exclusively low tones. Solely on the basis of such forms, we would have no way of knowing how forms with final high tones would behave; we would therefore presumably choose (as Leben did) the simplest rule that accounts for the facts, something like:

(37) L → H / $H]_{V\ N}[$___

This would result in the following derivations in the present tense:

(38) a. o de akpa → o de akpa
L H L L H H

b. o de ogede → o de ogede
L H L H L H H H

(38b) is incorrect, since the first e should bear low tone (see section 3 for discussion of this form). Thus, the prediction made by Leben's theory, while correct for the cases he discusses, is incorrect for others--even in the same language.

Leben's second argument involves a putative increase in simplicity allowed by maintaining this position in accounting for certain Mende data. Due to the existence of forms such as those mentioned above and other problems pointed out by Dwyer (1978), Leben (1978) has revised his earlier

analysis considerably. These revisions, it is argued, when coupled with a "convention on tone melodies" (p. 201) which deletes the second of a sequence of like tones (optionally separated by a word boundary), allow one to do without two rules which are apparently required by other analyses, as well as accounting for the problematic facts that Dwyer pointed out. The first revision involves allowing high tones to be associated lexically with a vowel in "exceptional words" (i.e. those that do not receive the correct phonetic tone pattern on the basis of Leben's mapping principles and his restricted set of underlying tone patterns, or otherwise behave unexpectedly from the standpoint of his analysis). Thus lèlèmá 'praying mantis' and ndàvúlá 'sling' would have the lexical representations given in (39), where the remaining association lines are added by Leben's principles (cf. Leben 1978:200):

(39) a. lelema b. ndavula

L H L H

Given such representations, forms like lèlèmá can be said to have an underlying LH melody rather than LLH, which is inexpressible in Leben's system. This revision also allows for a lexical distinction in the case of bisyllabic words with a L H melody between those in which there is no lexical assocaition of tones and those that have a high tone associated with the final syllable,[16] although such a distinction is not required to account for surface isolation tone patterns (only low-high is found). But it is required if we want to do without the rules alluded to above. Consider, for example, constructions involving the postposition -ma 'on', which is apparently underlyingly toneless, and is assigned the immediately preceding tone. If tones may be associated with more than one tone-bearing unit, there is no need for a Mende-specifc rule to account for such facts, since the (universal) Well-Formedness Condition predicts just this kind of behavior, as illustrated for nàvɔ́ 'money' (which has a lexically associated high tone in Leben's account) in (40):

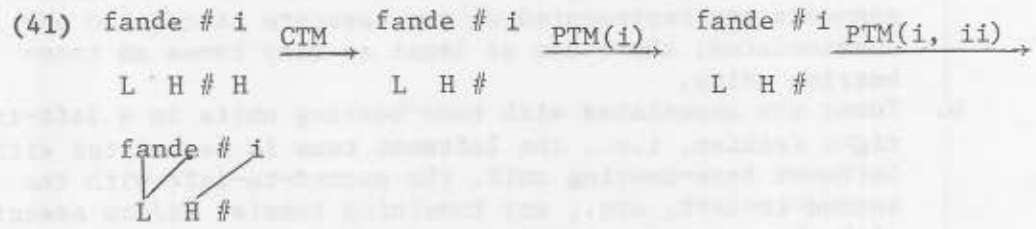
(40) navo - ma —WFC→ navo - ma

L H L H

Theories which require that tone be an ordinary segmental feature--and those that prohibit the association of a single tone with more than one tone-bearing unit--cannot account for these facts in this way. It is not clear, however, that this is a real problem for the present proposal. For one thing, toneless morphemes ar not always subject to "tone copying" as in this case; in a number of languages (cf. Hyman and Schuh 1974, Schuh 1978) such morphemes undergo "tone polarization", whereby they receive a tone opposite to a neighboring tone. That is, the universalist account is suspect, since the facts for which it purports to account are not in fact universal. If it should turn out that tone polarization is highly marked as a synchronic phenomenon (it is clearly not a natural diachronic tone rule in Hyman and Schuh's sense), then a universal convention which has the effect of copying a neighboring tone onto a toneless tone-bearing unit can be added to the theory. It might be objected that adding a new universal convention on top of those that make up the Well-Formedness Condition is somewhat ad hoc, but, as will be seen below, since much, if not all, of the

work done by the WFC is not necessary within the present framework, this new convention is not an additional one, but rather a (perhaps partial) replacement for the WFC. Thus, if automatic assignment of tones to toneless segments is desirable, it can be done even without appealing to the WFC.

It is also possible to account for the behavior of bisyllabic L H definite forms that do not have a lexically associated H (unlike the example in (40)) without a Mende-specific rule by making use of the convention on tone melodies (CTM) mentioned above and a revised set of principles of tone mapping (PTM), which are as follows (cf. Leben 1978: 200): (i) a final H is associated with the rightmost syllable; (ii) unassociated tones and segments are paired up from left to right; and (iii) a toneless syllable is associated with the tone of the preceding syllable. Any further association required is done according to the WFC. The operation of these principles is illustrated in (41) for fàndé 'cotton', where the high-toned í is the definite article:



That is, Leben's analysis accounts automatically for the fact that the final syllable in fàndé-like forms bears low tone when followed by a high tone. Other analyses would presumably require a Mende-specific rule to do this--either one that converts a H to L when it is between a L and a H, or one that simplifies a rising tone to low when followed by high (in analyses that treat fàndé-like forms as having, in essence, an underlying rising tone on the final syllable--cf. Dwyer 1978, Singler 1980, Szamosi et al. 1982). While it is true that such a language-specific rule is not necessary in Leben's account, it is also true that both CTM and PTM are themselves language-specific. That is, the cost of eliminating one rule (two in the case of the rising tone analyses, since the rising tone would have to be simplified to high when it is not followed by high) is adding two principles/conventions. Furthermore, Leben's account creates problems with respect to association of boundary symbols on the different tiers that Leben resolves (p. 202) by reassociating them on the basis of an unspecified principle (or set of principles) that require look-ahead global power (in that they refer whether a given reassociation **would** result in a new violation of WFC **if** it was performed), and, as Leben (1982) himself points out, his earlier account requires that at least one rule crucially precede the operations performed by the WFC--an impossibility in most versions of autosegmental theory. Thus, the Mende facts offer no support for the position that tones may be associated with more than one tone-bearing unit; neither do, to the best of my knowledge, any other natural language facts.

There is one final objection to the approach advocated here. It concerns the existence of contour tones on (phonetically) short vowels, which, Goldsmith (1976) argues, is the basis of a good argument against Leben's mapping proposal. Since there is almost incontrovertible evidence that such tones are composed of two (or more?) level tones (cf., for

example, Woo 1969, Leben 1973, Hyman and Schuh 1974), the existence of mapping entails that a **sequence** of tonal features is part of a single column of the matrix of phonological features. But this is impossible, at least if the mathematical sense of "matrix" is intended. A number of possible solutions to similar problems in the case of nontonal phenomena have been suggested (cf., for example, Krohn 1972, Campbell 1974b, Anderson 1976, and Herbert 1977), and the virtues of the present proposal are sufficiently in evidence that I feel it is worthwhile pursuing these suggestions (or others) with respect to the problem of contour tones, although I will not do so here for reasons of space.

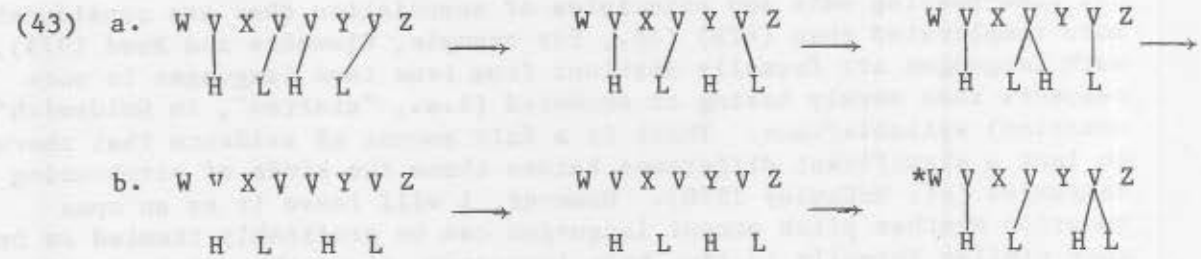
Having completed my discussion of objections to the revisions of autosegmental/suprasegmental theory suggested above, I would now like to make these revisions, which have so far been given only in rather broad outlines, more specific. The phonological component of the grammar of a true tone language is as follows:

(42) a. In the underlying representation, tones and "traditional" segments are represented on two separate tiers, and are unassociated; there are at least as many tones as tone-bearing units.
b. Tones are associated with tone-bearing units in a left-to-right fashion, i.e., the leftmost tone is associated with the leftmost tone-bearing unit, the second-to-left with the second-to-left, etc.; any remaining tone(s) is/are associated with the same tone-bearing unit as the tone to its/their left.
c. Rules that do not require mention of both tiers apply, subject to Clements and Ford's convention on reassociation of stranded tones (e.g., when vowels are deleted).
d. Tones are mapped onto tone-bearing units, i.e. they become features of segments in exactly the same sense as any other segmental feature.
e. Rules other than those in (c) apply.

Many aspects of this model have already been discussed, but other reasons for, and implications of, these requirements also deserve some attention. First of all, (42a, b) entail that contour tones in such languages will be found only morpheme-finally, unless they are derived by rule. Whether this is in fact the case is of course an empirical question, and, for the present, an open one. If it should turn out to be false, then the restrictions specified in (42a, b) will have to be relaxed somewhat; allowing for right-to-left association, for example, would permit morpheme-initial contour tones, and allowing some tones to be lexically associated would permit contour tones in any position. Since I know of no clear evidence that such a weakening of the theory is required, I will retain the stronger version as stated.[18]

Furthermore, condition (42b) might appear somewhat surprising--or even unnecessary--given the existence of (42d). Why not just **map** the tones in the first place (in left-to-right fashion), and do away with association completely? The answer is that doing so would create severe difficulties with respect to tonal stability when vowels are deleted (not assimilated). Note first of all that such deletions must take place prior to tonal mapping, since otherwise the tones would also be deleted. However, in order for Clements and Ford's convention to work, it is necessary to be

able to distinguish tones that have been "set afloat" from those that have not. Unless tones have been associated with tone-bearing units prior to vowel deletion, it is not clear how this could be done. This convention is necessary, furthermore, in order to insure appropriate reassociation, as in the hypothetical example in (43a), rather than inappropriate mapping, as in (43b):



(The final representation in (43b) would of course actually be in strictly segmental representation, but I will ignore this complication.) It thus appears that, however redundant having both association and mapping might seem, each plays a distinct, and necessary, role in the theory.

It is worth devoting some space to a discussion of the relationship between (42b) and Goldsmith's Well-Formedness Condition (cf. note ¿2). Given both (42a) and (42b), this condition is unnecessary, since it follows from them: all vowels must be associated with at least one vowel because of the requirements specified in (42a) and the first clause in (42b); all tones are associated with at least one vowel by the joint effect of the two clauses in (42b) (and what is more, with at most one vowel by (42a) and (42b) combined); and association lines may not cross by virtue of the (only) association procedure specified in (42b). That is, with the exception of the questionable automatic association of toneless segments discussed above, all of the work of the WFC is done by independently motivated principles of the theory advocated here--further evidence for this theory.

A further aspect of this proposal may strike some as somewhat suspicious: the second clause of (42a), especially when joined by (42d), makes it appear to be quite similar to a strictly segmental theory, and since representing tone directly as a segmental feature would allow us to do away with all of the conventions in (42), it might be suggested that tone should be represented in just this way. But the theory of "quasi-segmental" tonology advocated here has a number of advantages over a purely segmental theory. First of all, as pointed out above, it allows for a characterization of the differences between pre-mapping and post-mapping tone rules, and requires that the former universally precede the latter. More importantly, it accounts for the existence of tonal stability. As Goldsmith has pointed out, this phenomenon--which shows up both in "ordinary" linguistic phenomena such as vowel deletion and in the more unusual movements of tone-bearing units in language games such as those described in Hombert (1973) and Surintramont (1973)--is extremely problematic for strictly segmental theories. It thus appears that, although (underlying) tones in true tone languages are more closely related to the segments with which they are eventually associated than current non-segmental theories would have it, they are also less closely rleated to them than is maintained in strictly segmental aproaches.

One further point concerning the present proposal merits some discussion. This is the fact that it has been explicitly restricted to "true tone languages" (and, though not explicitly, to African languages). Nothing has been said about the treatment of "pitch accent" languages, which are formally quite similar to true tone languages in most current non-segmental theories. If these accounts of pitch accent are essentially correct, then, because they require the association of tones with more than one tone-bearing unit and principles of association that are considerably more complicated than (42b) (cf., for example, Clements and Ford 1979), such languages are formally distinct from true tone languages in more respects than merely having an accented (i.e., "starred", in Goldsmith's notation) syllable/tone. There is a fair amount of evidence that there is in fact a significant difference betwen these two kinds of pitch-using languages (cf. McCawley 1970). However, I will leave it as an open question whether pitch accent languages can be profitably treated as being more similar formally to true tone languages, since the existence of languages which appear to be intermediate types (cf. McCawley 1970, 1978; Voorhoeve 1973), at least suggests that the formal differences in question should not be as great as they now are. I will also not attempt to answer the question whether the distinction made by Pike (1948) between languages with a "register tone" system (typically, African and Amerindian languages) and those with a "contour tone" system (Asian languages) is a linguistically significant one, although the language game evidence at least suggests that it is (cf. Hombert 1976 and Churma 1979, ch. 5; but cf. also Yip (1980, 1982) for arguments that it is not). But the situation with respect to true tone languages seems much clearer. Here, tonal representations and rules must conform to the conditions specified in (42), which taken together constitute a theory of tonology that is significantly more restrictive than most other current theories. Moreover, previously proposed analyses allowed for by such less restrictive theories, but not by that advocated here, appear to be undesirable on independent grounds.

Footnotes

*An earlier version of sections 1 and 2 was presented at the Thirteenth Conference on African Linguistics under the title "On the diachronic development of tone in Etsako". I would like to thank Larry Hyman for helpful comments on that paper; Brian Joseph, David Stampe and Greg Stump also deserve a vote of thanks for discussion of the issues raised and/or comments on an earlier version of the full paper.

[1]The dialect described is that spoken in Ekpheli; there are twelve other dialects (cf. Elimelech 1978:2). Etsakọ is a Niger-Congo language of the Kwa subgroup (more narrowly, Ẹdo), and is spoken in Nigeria. (It is maintained by Elugbe (1980) that the proper name for the language is not Etsakọ--which is the name of the division in which the language is spoken--but Yẹkhee. Since my data come exclusively from Elimelech, I will use his term, but this should not be interpreted as an endorsement of it.)

I will adopt the following conventions for marking tones. High tone is indicated by an acute accent, low tone by a grace accent, falling tone by a circumflex, and rising tone by a hacek; "downstepped" high tone--a tone which behaves exactly like an "ordinary" high tone, except that it has a slighty lower pitch than a preceding high (or downstepped high) tone

(following high tones bear the same pitch as this downstepped high tone, and following lows show the same drop in pitch that they would show if they followed "ordinary" highs)--is indicated by preposing a tick before a syllable whose vowel has an acute accent. Dotted vowels are lax, and g is a voiced velar fricative.

[2]There is a general problem with the Etsakǫ data since, as Clements (1979) has pointed out, Elimelech's synchronic grammar "overgenerates". In particular, several rules described as being optional must apparently apply (or fail to apply) obligatorily for some forms, despite the fact that optional application correctly accounts for variant pronunciation in other cases. Since failure to record variant forms would not be an unexpected occurrence in a fieldwork situation, it is not clear whether this overgeneration is real or merely apparent, but it will have to be given some attention. Because of this problem, one cannot be certain about what those forms not given by Elimelech actually are, so I have been forced to make inferences based on the output of his rules, together with the behavior of other nouns in the same tense. Aside from the problem of optionality, I am reasonably certain about the correctness of the forms given.

[3]Elimelech (1974:63, 70) gives ǫ̀ dá'tásà, which is a further indication that the overgeneration problem may be more apparent than real, since this form can be obtained by applying one of Elimelech's "optional" rules to the form given in his later work.

[4]Elimelech (1978:93-102) treats these morphemes are having underlying falling tones (or high plus a "floating" low tone) in the affirmative. While such an account has some synchronic support in the case of the future and the past habitual, the only reason for maintaining it for the past perfect appears to be preserving the "generalization" that negatives are characterized by "a complete tone reversal of any aspectual morphemes..." (i.e., falling in the affirmative, rising in the negative) in their underlying form. Since there are only three tenses involved, proposing an abstract analysis solely in order to bring one tense into line with the (two) remaining tenses is highly suspect, and I will not attempt a diachronic explanation of this putative generalization.

[5]Elimelech gives no explicit discussion of the development of the development of the customary (although he does give a schematic reconstruction (p. 70)), but it is clear that an account like that in (3) is intended. My version of the development of the past is somewhat simplified, in that Elimelech (pp. 65-6) treats the two vowel deletions as occurring at separate stages; nothing here depends on whether this is in fact the case.

[6]As noted above, Elimelech (1978) refers to what he earlier called simply "present" as "present progressive". It appears that his earlier description was more accurate, since Etsakǫ has no "past progressive", although it does have both a present and a past habitual. That is, there is a separable habitual morpheme (at least semantically--it does not seem possible to give a unique phonological form for this morpheme), but there is no evidence for a morpheme which can be interpreted as progressive aspect. There are exceptions to the generalization mentioned, including

present day Etsako, so the violation of this statistical universal can be only suggestive and not conclusive.

[7]Elimelech does not state, either formally or informally, exactly which structures the rule of high tone spread is meant to affect, although it is clear that it must affect only nouns composed exclusively of low tones when they follow a (high toned) verb in the present tense converting all of the low tones to high. (No noun which contains a high tone is affected--see Table 1.) Elimelech (p. 73) suggests that this is evidence that low tone is a feature of the word (rather than each tone-bearing unit) in the case of low-low-(low) words, since "high spreads over one low tone only". The evidence for the latter claim is the behavior of words like àtásà, which shows up later as á'tásà in the present, as compared with something like ọ̀gèdẹ̀, which appears as ọ́gèdẹ̀. But the high tone on the initial vowel in these forms has nothing to do with "high tone spread", as evidenced by the appearance of initial high tone (sometimes downstepped) in **all** tenses, and not just in the present as one would expect if high spread operated to prevent the present from merging with the customary. (The high tone in these latter cases is pretty clearly the effect of the loss of the vowel of the high toned verb stem.) Thus, the "sound change" in question must make reference not only to specific tenses (or perhaps a preceding tautosyllabic low tone--although this has nothing to do with preventing merger) but also to whether or not a word contains exclusively low tones (or, putatively, has low tone as a word-level feature).

[8]The universal in question is claimed in these works to be valid only synchronically. Clements and Ford in fact argue (cf. also Hyman and Tadadjeu 1976) that for synchronic "floating" tones, which presumably have the same diachronic source as other stranded tones, the direction of association is unpredictable. While this is undoubtedly true, I would maintain that this lack of predictability would not carry over to a fully diachronic account, since the apparent counterexamples to the diachronic analogue of this universal should be amenable to explanation as being the result of tonal changes (e.g., spreading, in the sense of Hyman and Schuh) which operated prior to the loss of the tone-bearing element, as in the account presented in the following subsection. For discussion of a related concern, see section 4.

[9]It might be supposed that the necessity of positing (8) indicates that (6d) is rather suspicious. However, there is some fairly strong evidence that the latter did in fact occur. If it did, then for word-internal tonal sequences *L H H ..., we should get *L L͡H H... While the latter do not occur, neither, with the exception of two forms out of the 542 listed in Elimelech's appendix (and one cited in the text but not listed there), do the former. This near gap could be explained if (6d) had occurred, and was followed by another change simplifying L͡H to L before H (cf. (6b)). Furthermore, the two forms in question are themselves suspicious in that one (ọ̀gbédẹ̀ 'morning') does not appear to be cognate with five out of the seven other dialects of Etsako represented in Elimelech's appendix, and that the other (ọ̀kấíkấì 'gin') is clearly reduplicative in nature and is the kind of lexical item that could quite well be borrowed (note that no forms are given for two of the dialects for this item). While no comparative information is available for ọ̀tsẹ́dẹ̀ 'sunrise', it is not unlikely that it too is a relatively late borrowing.

[10]It is also possible to offer an alternative account of the origin of the initial low tone on the verb-noun sequence in the case of the present tense, namely that the present has no overt marker, and that the low tone thus is spread from the pronoun onto the verb. (This would, of course, require a boundary adjustment in the reconstructed forms in (7).) Although this possibility cannot be entirely ruled out, it seems rather unlikely. If this were so, then the present and the past would be expected to merge, since (6a) would convert the past tense morpheme to v̲, which would then be simplified to v̂ by (6b), thus not allowing derivation of the rising tone on the pronoun. As far as I can see, the only way to prevent this merger is to claim that low-spreading is blocked in the past tense in order to prevent merger with the habitual. This would require in addition that the vowel qualities of the past and the habitual markers be identical.

[11]Since high tones are automatically downstepped (i.e., "downdrifted") after a low tone, it is not really necessary to specify the following high as being downstepped, as long as (15e) does not precede the rule of downdrift, as seems to be universally the case (cf. Churma 1982). Because the latter rule would add irrelevant details to the derivations, I indicate its application only in cases of non-automatic (contrastive) downstep.

[12]This condition contains the following provisions (Goldsmith 1976:27): (i) all vowels are associated with at least one tone; (ii) all tones are associated with at least one vowel; and (iii) association lines do not cross. Together with the apparently universal convention (it is at least unmarked; Clements and Ford 1979, argue rather unconvincingly that it must violated in Kikuyu) that leftmost tones are associated with leftmost tone-bearing units, the WFC will correctly specify the tone patterns given as the output of (21) in the derivatoins in (22) below.

[13]No explanation has been given for the invariably obligatory nature of pre-mapping rules, but it seems that the mapping proposal will undobtedly play a large part in such an explanation: given that sound change affects post-mapping reprsentations, and assuming further that only posible sound changes may have synchronic analogues that are optional, it follows that only post-mapping rules may be optional, and thus that pre-mapping rules are necessarily obligatory. Pre-mapping rules, on this view, are the result of "telescoping" two or more diachronic changes (cf. also Hyman and Schuh 1974).

[14]This account draws heavily on the work of David Stampe and his students (cf. especially Semiloff-Zelasko 1973, although her account is of course concerned with a somewhat different phenomenon). Dwyer (ms) has, apparently independently, come to quite similar conclusions. I believe that it is possible to make a stronger claim than that made by these authors, namely, that **all** (or almost all--cf. note 16) apparent deletions (in diachrony and in "natural" synchrony) are due to assimilation, although supporting such a claim is beyond the scope of this paper. Kay Williamson, in her first paper in George (1972), gives an analysis of a case of tonal stability that presupposes an approach to this pheomenon which is essentially identical to that advocated here, but gives no arguments to support it; in her later paper in the same volume, she makes explicit

theoretical claims which preclude her earlier analysis, but are falsified by the data given there (as well as other cases of stability).

[15]Oral cavity features need not be totally assimilated, either. Of special interest here are cases in which the first vowel is assimilated to the second with respect to some oral cavity features, and the latter is assimilated to the former with respect to the remaining features, thus giving the appearance of a third (long) vowel that is a "coalescence" of the two vowels in question. This phenomenon is quite common; Sanskrit is one well-known example of a language with such a process (e.g., a + i → e:, a + u → o:).

[16]Not all cases of apparent compensatory lengthening are the result of such a set of events. DeChene and Anderson (1979) discuss another common source, and examples such as Kikuyu /mo-aná/ 'child' → [mwa:ná] (cf. Clements and Ford 1981:318), which presumably mirrors exactly the diachronic development of this form, indicate that there is (at least) another. In the latter case, the explanation appears to be that the moraic structure of words is maintained even when there is a devocalization, reflected in this case by the retention of the original two morae in the lengthened a:. Length, unlike tone, is truly autosegmental (in that it is never mapped onto segments). Such examples show that the claim made by deChene and Anderson that apparent compensatory lengthening is always "composite" cannot be maintained, although they are correct that many cases described otherwise are in fact composite (as in those discussed here).

[17]As Leben notes, this account entails the existence of an "accidental gap", in that there are no bisyllabic LH words with the H associated lexically with the first (rather than the second) syllable. Since there are also no trisyllabic words which require a lexical first-syllable H, one might well question the acidental nature of this gap. The fact that all trisyllabic words have a lexically associated H is further evidence that Leben's account is considerably less than optimal.

[18]Clements and Ford's (1979) analysis of Kikuyu, which employs a rule that associates the leftmost tone with the tone-bearing segment that is second from the left, would be, if accepted, a counterexample to even the weakest version of these conditions. There is considerable evidence, however, that it should not be accepted. For one thing, it is exceedingly abstract, in the sense of Kiparsky (1973), and there are other problems with Clements and Ford's general framework which I have discussed elsewhere (cf. Churma 1982).

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